

(3.) Another, better, method was used. Two frogs, whose brains had been destroyed as before, were taken, and into the dorsal lymphatic sac of each a small drop of 5 per cent. strychnine was injected. A minute afterwards only 0·1 cub. centim. of 10 per cent.  $\beta$  lutidine was injected into one frog (A), the other (B) remaining as before.

After fifteen minutes (A) gave no signs of strychnine tetanus, while (B) gave distinct signs. After twenty minutes (A) gave only very faint reflex action; but (B), on being touched, went into strong tetanus. After thirty minutes (A) gave no signs of reflex action, while (B) went into strong tetanic convulsions on simply touching the table.

These results lasted over an hour; then into (B) 0·1 cub. centim. of 10 per cent.  $\beta$  lutidine was injected, and in ten minutes the effect of the strychnine began to pass off, and in thirty minutes was quite gone, the frog not even giving signs of reflex action.

Again, in some experiments strychnine was injected into one frog (prepared as before), and strychnine with  $\beta$  lutidine into another. After twenty-four hours the first went into strong tetanic convulsions on touching, but never the latter; that of the former disappearing after the injection of the alkaloid.

From our experiments we hope we have made it clear that  $\beta$  lutidine causes a distinct increase in the tonicity of both cardiac and voluntary muscular tissue, also a slowing in the rate of the heart's beat, and that it arrests the inhibitory power of the vagus. That by its action upon the nerve cells of the spinal cord, it, in the first place, lengthens the time of reflex action, and then arrests that function; finally, that it is successfully antagonistic to strychnine in its action upon the spinal cord.

In conclusion, we feel much pleasure in acknowledging our grateful thanks to Dr. Michael Foster, both for his kind help and happy suggestions, which have been of great assistance to us in our investigations.

## II. "Discussion of the Results of some Experiments with Whirled Anemometers." By Professor G. G. STOKES, Sec. R.S. Received April 26, 1881.

In the course of the year 1872, Mr. R. H. Scott, F.R.S., suggested to the Meteorological Committee the desirability of carrying out a series of experiments on anemometers of different patterns. This suggestion was approved by the Committee, and in the course of same year a grant was obtained by Mr. Scott from the Government Grant administered by the Royal Society, for the purpose of defraying the expenses of the investigation. The experiments were not, however,

carried out by Mr. Scott himself, but were entrusted to Mr. Samuel Jeffery, then Superintendent of the Kew Observatory, and Mr. G. M. Whipple, then First Assistant, the present Superintendent.

The results have never hitherto been published, and I was not aware of their nature till on making a suggestion that an anemometer of the Kew standard pattern should be whirled in the open air, with a view of trying that mode of determining its proper factor, Mr. Scott informed me of what had already been done, and wrote to Mr. Whipple, requesting him to place in my hands the results of the most complete of the experiments, namely, those carried on at the Crystal Palace, which I accordingly obtained from him. The progress of the enquiry may be gathered from the following extract from Mr. Scott's report in returning the unexpended balance of the grant.

"The comparisons of the instruments tested were first instituted in the garden of the Kew Observatory. This locality was found to afford an insufficient exposure.

"A piece of ground was then rented and enclosed within the Old Deer Park. The experiments here showed that there was a considerable difference in the indications of anemometers of different sizes, but it was not possible to obtain a sufficient range of velocities to furnish a satisfactory comparison of the instruments. Experiments were finally made with a rotating apparatus, a steam merry-go-round, at the Crystal Palace, which led to some results similar to those obtained by exposure in the Deer Park.

"The subject has, however, been taken up so much more thoroughly by Drs. Dohrandt and Thiesen (*vide* "Repertorium für Meteorologie," vols. iv and v) and by Dr. Robinson in Dublin, that it seems unlikely that the balance would ever be expended by me. I, therefore, return it with many thanks to the Government Grant Committee.

"The results obtained by me were hardly of sufficient value to be communicated to the Society."

On examining the records, it seemed to me that they were well deserving of publication, more especially as no other experiments of the same kind have, so far as I know, been executed on an anemometer of the Kew standard pattern. In 1860 Mr. Glaisher made experiments with an anemometer whirled round in the open air at the end of a long horizontal pole,\* but the anemometer was of the pattern employed at the Royal Observatory, with hemispheres of 3·75 inches diameter and arms of 6·725 inches, measured from the axis to the centre of a cup, and so was considerably smaller than the Kew pattern. The experiments of Dr. Dohrandt and Dr. Robinson

\* "Greenwich Magnetical and Meteorological Observations," 1862, Introduction, p. li.

were made in a building, which has the advantage of sheltering the anemometer from wind, which is always more or less fitful, but the disadvantage of creating an eddying vorticosc movement in the whole mass of air operated on; whereas in the ordinary employment of the anemometer the eddies it forms are carried away by the wind, and the same is the case to a very great extent when an anemometer is whirled in the open air in a gentle breeze. Thus, though Dr. Robinson employed among others an anemometer of the Kew pattern, his experiments and those of Mr. Jeffery are not duplicates of each other, even independently of the fact that the axis of the anemometer was vertical in Mr. Jeffery's and horizontal in Dr. Robinson's experiments; so that the greater completeness of the latter does not cause them to supersede the former.

In Mr. Jeffery's experiments the anemometers operated on were mounted a little beyond and above the outer edge of one of the steam merry-go-rounds in the grounds of the Crystal Palace, so as to be as far as practicable out of the way of any vortex which it might create. The distance of the axis of the anemometer from the axis of the "merry" being known, and the number of revolutions ( $n$ ) of the latter during an experiment counted, the total space traversed by the anemometer was known. The number ( $N$ ) of *apparent* revolutions of the anemometer, that is, the number of revolutions *relatively to the merry*, was recorded on a dial attached to the anemometer, which was read at the beginning and end of each experiment. As the machine would only go round one way, the cups had to be taken off and replaced in a reverse position, in order to reverse the direction of revolution of the anemometer. The *true* number of revolutions of the anemometer was, of course,  $N+n$ , or  $N-n$ , according as the rotations of the anemometer and the machine were in the same or opposite directions.

The horizontal motion of the air over the whirling machine during any experiment was determined from observations of a dial anemometer with 3-inch cups on 8-inch arms, which was fixed on a wooden stand in the same horizontal plane as that in which the cups of the experimental instrument revolved, at a distance estimated at about 30 feet from the outside of the whirling frame. The motion of the centres of the cups was deduced from the readings of the dial of the fixed anemometer at the beginning and end of each experiment, the motion of the air being assumed as usual to be three times that of the cups.

The experiments were naturally made on fairly calm days, still the effect of the wind, though small, is not insensible. In default of further information, we must take its velocity as equal to the mean velocity during the experiment.

Let  $V$  be the velocity of the anemometer (*i.e.*, of its axis),  $W$  that of the wind,  $\theta$  the angle between the direction of motion of the

anemometer and that of the wind. Then the velocity of the anemometer relatively to the wind will be—

$$\sqrt{V^2 - 2VW \cos \theta + W^2} \dots \dots \quad (a)$$

The mean effect of the wind in a revolution of the merry will be different according as we suppose the moment of inertia of the anemometer very small or very great.

If we suppose it very small, the anemometer may be supposed to be moving at any moment at the rate due to the relative velocity at that moment, and therefore the mean velocity of rotation of the cups in one revolution of the merry will be that corresponding to the mean relative velocity of the anemometer and the air. If, as is practically the case,  $W$  be small as compared with  $V$ , we may expand (a) in a rapidly converging series according to ascending powers of  $W$ . All the odd powers will disappear in taking the mean, and if we neglect the fourth and higher powers we shall have for the mean

$$V + \frac{W^2}{4V},$$

so that  $W^2/4V$  is the small correction to be added to the measured velocity of the anemometer in order to correct for the wind.

On the other hand, if the moment of inertia of the anemometer be taken as very great, the rate of rotation of the cups during a revolution of the merry will be sensibly constant. If  $V'$  be the velocity of the anemometer relatively to the air,  $v$  the velocity of the centre of one of the cups, and if we suppose the rotation of the anemometer resisted by a force of which the moment is  $F$ , then, according to Dr. Robinson's researches, we have approximately

$$F = AV'^2 - 2BvV' - Cv^2.$$

In the present case friction is not taken into account, and instead of  $F$  we must take the moment of the effective moving force. Furthermore, it appears from the experiments of Dr. Robinson, in Dublin, that the observations were almost as well satisfied by taking the first two terms only of the above expression for  $F$  as by taking all three, and this simplification may be employed with abundantly sufficient accuracy in making the small correction for the wind. We have, therefore—

$$F = AV'^2 - 2BvV',$$

where  $V'$  is given by (a). In order that the anemometer may be neither accelerated nor retarded from one revolution of the merry to another, the mean effective force must be *nil*; and taking the means of both sides of the above equation, observing that, in consequence of the supposed largeness of the moment of inertia,  $v$  is

sensibly constant during one revolution of the merry, we have on employing the approximate value of the mean of  $V'$  or (*a*) already used—

$$0 = A(V^2 + W^2) - 2Bv\left(V + \frac{W^2}{4V}\right).$$

But if  $U$  be the constant velocity of air relatively to the anemometer which would make the cups turn round at the same rate, we have similarly—

$$0 = AU^2 - 2BvU.$$

Eliminating  $Bv/A$ , between these two equations we get—

$$U\left(V + \frac{W^2}{4V}\right) = V^2 + W^2 \quad \dots \dots \dots \quad (c),$$

and as the fourth and higher powers of  $W$  have been neglected all along, we get from the last—

$$U = V + \frac{3W^2}{4V} \quad \dots \dots \dots \quad (d),$$

so that, on this supposition, the mean correction for the wind is  $3W^2/4V$ , or three times the correction of the former supposition.

The mean value of the radical (*a*) is given by an elliptic function; but even in an extreme case among the experiments, when the ratio of the velocity of the wind to that of the anemometer is as great as 3 to 5, the error of the approximate expression  $V + W^2/4V$  amounts only to about 0·01 mile an hour, which may be quite disregarded. The error in employing (*d*) for the determination of  $U$  instead of (*c*) is of about similar amount.

Three anemometers were tried, namely, one of the old Kew standard pattern, one by Adie, and Kraft's portable anemometer. Their dimensions will be found at the heads of the respective tables below. With each anemometer the experiments were made in three groups, with high, moderate, and low velocities respectively, averaging about 28 miles an hour for the high, 14 for the moderate, and 7 for the low. Each group again was divided into two subordinate groups, according as the cups were direct, in which case the directions of rotation of the merry and of the anemometer were opposite, or reversed, in which case the directions of the two rotations were the same.

The data furnished by each experiment were: the time occupied by the experiment, the number of revolutions of the merry, the number of *apparent* revolutions of the anemometer, given by the difference of readings of the dial at the beginning and end of the experiment, and the space  $S$  passed over by the wind, deduced from the difference of readings of the fixed anemometer at the beginning and end of the experiment.

The object of the experiment was, of course, to compare the mean velocity of the centres of the cups with the mean velocity of the air relatively to the anemometer. It would have saved some numerical calculation to have compared merely the spaces passed through during the experiment; but it seemed better to exhibit the velocities in miles per hour, so as to make the experiments more readily comparable with one another, and with those of other experimentalists. In the reductions I employed 4-figure logarithms, so that the last decimal in  $V$  in the tables cannot quite be trusted, but it is retained to match the correction for  $W$ , which it seemed desirable to exhibit to 0·01 mile.

On reducing the experiments with the low velocities, I found the results extremely irregular. I was subsequently informed by Mr. Whipple, that the machine could not be regulated at these low velocities, for which it was never intended, and that it sometimes went round fast, sometimes very slowly. He considered that the experiments in this group were of little, if any, value, and that they ought to be rejected. They were besides barely half as numerous as those of the moderate group. I have accordingly thought it best to omit them altogether.

In the following tables the first column gives the group,  $H$  standing for high velocities,  $M$  for moderate; the subordinate group, — standing for rotation of the anemometer opposite to that of the machine, + for rotations in the same direction, and lastly the reference number of the experiment in each subordinate group.  $T$  gives the duration of the experiment in minutes;  $n$  the number of revolutions of the machine;  $N$  the number of *apparent* revolutions of the anemometer;  $S$  the space passed over by the natural wind, in miles. These form the data. From them are calculated:  $V$ , the velocity of the anemometer, in miles per hour;  $W$ , the velocity of the wind;  $W^2/2V$  the mean of the two corrections to be added to  $V$  on account of the wind, according as we adopt one or other of the extreme hypotheses as to the moment of inertia of the anemometer, namely, that it is very small or very large. The actual correction will be half the number in this column on the first supposition, and once and a-half on the second.  $V_1, V_2$  denote the velocity of the anemometer, or, in other words, of the artificial wind, corrected for the natural wind on these two suppositions respectively, so that the last two columns give 100 times the ratio of the registered velocity to the true velocity, or the registered as a percentage of the true, the registered velocity meaning that deduced from the velocity of the cups on employing the usual factor 3.

The dials of the first two anemometers read only to 10 revolutions, which is the reason why all the numbers  $N$  end with a 0.

## The Old Kew Standard.

Diameter of Arms between Centres of Cups 48 inches; Diameter of Cups 9 inches. Fixed to Machine at 22·3 feet from the Axis of Revolution.

Group and No.	T.	n.	N.	S.	V.	W.	$\frac{W^2}{2V}$	$\frac{300v}{V_1}$	$\frac{300v}{V_2}$
H -									
1	15	303	1690	0·7	31·17	2·80	0·13	126·9	126·3
2	18	301	1690	0·5	26·63	1·67	0·05	124·1	123·8
3	16	301	1580	0·6	29·96	2·25	0·08	114·2	113·9
4	17	300	1710	1·1	23·11	3·88	0·27	125·9	124·7
5	17	300	1720	1·1	28·11	3·88	0·27	126·8	125·5
6	22	400	2210	1·3	28·96	3·27	0·18	121·4	120·6
7	23	400	2220	1·1	27·70	2·87	0·15	122·1	121·5
8	19	300	1670	0·7	25·14	2·21	0·10	122·6	122·2
9	19	300	1640	0·8	25·14	2·53	0·13	119·8	119·3
10	17	301	1670	0·8	28·20	2·71	0·13	122·1	121·5
11	19	300	1670	0·8	25·14	2·53	0·13	122·6	122·0
Mean ...	...	...	...	...	26·75	2·78	0·15	122·6	121·9
H +									
1	17	302	980	0·0	28·29	0·00	0·00	114·2	114·2
2	17	300	960	1·0	28·11	3·53	0·22	112·6	111·7
3	15½	300	1000	1·4	30·82	5·42	0·48	115·7	113·9
4	22½	300	1080	1·8	21·23	4·80	0·54	122·3	119·2
5	19	300	1020	0·7	25·14	2·21	0·10	118·1	117·7
6	16	300	1030	0·9	29·86	3·37	0·19	119·0	118·2
7	18	300	1050	0·8	26·54	2·67	0·13	120·8	120·2
8	18	301	1060	0·6	26·63	2·20	0·09	121·4	121·0
9	18	300	1000	0·7	26·54	2·43	0·11	121·7	121·3
Mean ...	...	...	...	...	27·02	2·96	0·21	118·4	117·5
M -									
1	30	300	1650	0·8	15·92	1·60	0·08	120·8	120·2
2	31	300	1670	1·6	15·41	3·10	0·31	121·7	119·3
3	34	300	1570	1·9	14·05	3·01	0·22	112·6	110·1
4	36	300	1540	1·7	13·26	2·83	0·30	110·0	107·6
5	36	300	1540	1·3	13·26	2·17	0·18	110·5	109·0
Mean ...	...	...	...	...	14·38	2·54	0·24	115·1	113·2
M +									
1	28	301	880	0·0	17·63	0·00	0·00	102·6	102·5
2	38	300	940	2·0	12·57	3·15	0·38	109·5	106·4
3	38	300	890	1·6	12·57	2·52	0·25	105·7	103·7
4	36	300	990	0·8	13·26	1·33	0·07	115·4	114·9
5	35	300	990	1·0	13·66	1·71	0·07	115·3	114·8
Mean ...	...	...	...	...	13·94	1·74	0·15	109·7	108·5

## Adie's Anemometer.

Diameter of Arms between Centres of Cups 13·4 inches; Diameter of Cups 2·5 inches. Fixed to Machine at 20·7 feet from the Axis of Revolution.

Group and No.	T.	n.	N.	S.	V.	W.	$\frac{W^2}{2V}$	$\frac{300v}{V_1}$ .	$\frac{300v}{V_2}$ .
H-									
1	17	300	3860	1·0	26·16	3·53	0·24	95·2	94·4
2	15 $\frac{1}{2}$	300	3650	1·4	28·61	5·45	0·52	89·5	87·9
3	22 $\frac{1}{2}$	300	3940	1·8	19·70	4·80	0·58	96·7	94·0
4	19	300	3760	0·7	23·33	2·21	0·10	93·1	92·7
5	16	300	3780	0·9	27·71	3·37	0·20	93·5	92·8
6	18	300	3890	0·9	24·63	2·67	0·14	96·4	96·0
7	18	301	3980	0·7	24·72	2·33	0·11	98·6	98·2
8	18	300	3940	0·8	24·63	2·67	0·14	97·8	97·3
Mean...	..	..	..	..	24·94	3·38	0·25	95·1	94·2
H+									
1	17	300	3240	1·1	26·08	3·89	0·29	94·9	93·9
2	17	300	3330	1·1	26·08	3·89	0·29	97·3	96·3
3	19	300	3760	0·7	23·33	2·21	0·10	109·2	108·7
4	16	300	3780	0·9	27·71	3·37	0·20	109·6	108·8
5	19	300	3060	0·8	23·33	2·53	0·14	90·3	89·8
6	17	301	3120	0·8	26·17	2·82	0·15	91·6	91·1
7	19	300	3160	0·8	23·33	2·53	0·14	93·0	92·5
Mean...	..	..	..	..	25·15	3·03	0·19	98·0	97·3
M-									
1	38	300	3620	2·0	11·67	3·16	0·43	87·7	84·9
2	38	300	3500	1·6	11·67	2·53	0·43	84·7	81·7
3	36	300	3910	0·8	12·26	1·33	0·07	97·5	96·9
4	35	300	3430	1·0	12·67	1·71	0·07	84·1	83·7
Mean...	..	..	..	..	12·07	2·18	0·25	88·5	86·8
M+									
1	31	300	3250	1·6	14·30	3·10	0·34	94·3	93·7
2	34	300	2920	1·7	13·04	3·00	0·35	85·7	85·5
3	34	300	2940	1·9	13·04	3·06	0·36	86·1	83·1
4	36	300	2760	1·7	12·31	2·83	0·33	81·4	79·2
5	36	300	2780	1·3	12·31	2·17	0·19	65·5	64·7
Mean...	..	..	..	..	13·00	2·83	0·31	82·6	81·0

## Kraft's Portable Anemometer.

Diameter of Arms between Centres of Cups 8·3 inches; Diameter of Cups 3·3 inches. Fixed to Machine at 19·10 feet from the Axis of Revolution.

Group and No.	T.	n.	N.	S.	V.	W.	$\frac{W^2}{2V}$	$\frac{300v}{V_1}$	$\frac{300v}{V_2}$
H -									
1	19	300	5594	0·6	21·53	1·89	0·08	95·6	95·4
2	17½	303	5681	0·7	23·60	2·69	0·15	96·2	95·4
3	15	303	5990	0·7	27·22	2·80	0·14	102·8	102·4
4	18	301	6086	0·5	22·79	1·67	0·06	104·2	104·0
5	16	301	6116	0·6	25·65	2·25	0·10	104·7	104·3
6	17	300	6143	1·1	24·06	3·88	0·31	105·2	103·6
7	17	300	6240	1·1	24·79	3·27	0·21	101·4	100·4
8	22	400	7896	1·3	24·79	3·27	0·16	101·5	100·9
9	23	400	7900	1·1	23·71	2·78	0·11	102·3	101·7
10	19	300	5966	0·7	21·53	2·21	0·11	98·3	97·7
11	19	300	5751	0·8	21·53	2·53	0·15	99·6	99·0
12	17	301	5842	0·8	24·14	2·82	0·16	100·9	100·1
13	19	300	5892	0·8	21·53	2·53	0·15	100·9	100·1
Mean...	...	...	...		23·55	2·71	0·16	101·5	100·8
H +									
1	17	300	5372	1·0	24·06	3·53	0·26	102·2	101·0
2	15½	300	5265	1·4	26·39	5·42	0·56	99·7	97·5
3	22½	300	5460	1·8	18·18	4·80	0·63	102·5	98·9
4	19	300	5093	0·7	21·53	2·21	0·11	97·4	96·8
5	16	300	5282	0·9	25·57	3·37	0·22	100·6	99·8
6	18	300	5274	0·8	22·72	2·67	0·16	100·6	99·8
7	18	301	5300	0·6	22·79	2·00	0·09	100·9	100·5
8	18	300	5363	0·8	22·72	2·67	0·16	102·2	101·4
Mean...	...	...	...		22·99	3·33	0·27	100·8	99·4
M -									
1	30	300	5488	0·8	13·63	1·60	0·09	93·3	92·5
2	31	300	5880	1·6	13·19	3·10	0·36	99·2	96·6
3	34	300	5168	1·6	12·03	2·53	0·27	86·8	84·8
4	34	300	5320	1·9	12·03	3·35	0·47	88·8	85·4
5	36	300	5030	1·7	11·36	2·83	0·35	84·0	81·4
6	36	300	4910	1·3	11·36	2·17	0·21	82·3	80·9
Mean...	...	...	...		12·27	2·60	0·29	89·1	86·9
M +									
1	38	300	4508	2·0	10·76	3·16	0·46	84·8	81·4
2	38	300	4250	1·6	10·76	2·53	0·30	80·9	78·7
3	36	300	5006	0·8	11·36	1·33	0·08	95·3	94·7
4	35	300	4743	1·0	11·69	1·71	0·12	90·3	89·3
Mean...	...	...	...		11·14	2·18	0·24	87·8	86·0

The mean results for the high and moderate velocities, contained in

the preceding tables, are collected in the following table, in which are also inserted the mean errors.

Anemometer.		Directions of Rotation.	High Velocities.				Moderate Velocities.			
			Mom. inert. small.		Mom. inert. large.		Mom. inert. small.		Mom. inert. large.	
			p. c.	m. e.	p. c.	m. e.	p. c.	m. e.	p. c.	m. e.
Kew.	Opposite . . . .	122·6	2·4	121·9	2·3	115·1	4·9	113·2	5·2	
		118·4	2·9	117·5	2·8	109·7	4·5	108·5	5·1	
		Mean . . . .	120·5	..	119·7	..	112·4	..	110·8	
Adie.	Opposite . . . .	95·1	2·3	94·2	2·3	88·5	4·5	86·8	5·0	
		98·0	6·5	97·3	6·5	82·6	7·3	81·0	7·3	
		Mean . . . .	96·5	..	95·7	..	85·5	..	83·9	
Kraft.	Opposite . . . .	101·5	2·6	100·8	2·5	89·1	4·8	86·9	5·1	
		100·8	1·2	99·4	1·3	87·8	5·0	86·0	6·0	
		Mean . . . .	101·1	..	100·1	..	88·4	..	86·4	

The mean errors exhibited in the above table show no great difference according as we suppose the moment of inertia of the anemometer small or large in correcting for the wind. There appears to be a slight indication, beyond what may be merely casual, that the errors are a little greater on the latter supposition than on the former, which is what we should rather expect; for an anemometer would get pretty well under way in a fraction of a revolution of the whirling instrument. However, the difference is so small that it will suffice to take the mean of the two as the mean error belonging to the particular anemometer, class of velocity, and character of rotation under consideration. From the mean errors we may calculate nearly enough, by the usual formulæ, the probable errors of the various mean percentages for rotations opposite and alike. The probable errors of these mean percentages come out as follows:—

Kew, 1·0 for high velocities; 2·7 for moderate velocities.

Adie, 1·5 " " 2·0 " "

Kraft, 0·9 " " 1·8 " "

These probable errors are so small that it appears that for the high and even for the moderate velocities the experiments are extremely trustworthy, except in so far as they may be affected by *systematic* sources of error.

If we compare the registered percentages of the true velocity of

the air relatively to the anemometer according as the rotations are in opposite directions or in the same direction, we see that in five out of the six cases they are slightly greater when the rotations are opposite. The sole exception is in the group "Adie, high velocities," which is made up of the groups "Adie H—" and "Adie H+." On referring to the principal table for the Adie, we see that Experiments 3 and 4 in group H+ give percentages usually high, depending on the high values of N. These raise the mean for the group, and make the mean error far greater than those of the other five groups for high velocities. There appears little doubt, therefore, that the excess of percentages obtained for rotations opposite is real, and not merely casual. It is, however, so small as to give us much confidence in the correctness of the mean result, unless there were causes to vitiate it which apply to both directions of rotation alike.

It may be noticed that the difference is greatest for the Kew, in which the ratio of  $r$  to R is greatest,  $r$  denoting the radius of the arm of the anemometer, and R the distance of its axis from the axis of revolution of the machine, and appears to be least (when allowance is made for the two anomalous experiments in the group "Adie H+") for the Kraft, for which  $r/R$  is least. In the Kraft, indeed, the differences are roughly equal to the probable errors of the means. In these whirling experiments  $r/R$  is always taken small, and we might expect the correction to be made on account of the finiteness of R to be expressible in a rapidly converging series according to powers of  $r/R$ , say—

$$A' \frac{r}{R} + B' \left( \frac{r}{R} \right)^2 + C' \left( \frac{r}{R} \right)^3 + \dots$$

We may, in imagination, pass from the case of rotations opposite to that of rotations alike, by supposing R taken larger and larger in successive experiments, altering the angular velocity of revolution so as to preserve the same linear velocity for the anemometer, and supposing the increase continued until R changes sign in passing through infinity, and is ultimately reduced in magnitude to what it was at first. The ideal case of  $R=\infty$  is what we aim at, in order to represent the motion of a fixed anemometer acted on by perfectly uniform wind by that of an anemometer uniformly impelled in a rectilinear direction in perfectly still air. We may judge of the magnitude of the leading term in the above correction, provided it be of an odd order, by that of the difference of the results for the two directions of rotation. Unless, therefore, we had reason to believe that A' were 0, or at least very small compared with B', we should infer that the whole correction for the finiteness of R is very small, and that it is practically eliminated by taking the mean of the results for rotations opposite and rotations alike.

We may accept, therefore, the mean results as not only pretty well freed from casual irregularites which would disappear in the mean of an infinite number of experiments, but also, most probably, from the imperfection of the representation of a rectilinear motion of the anemometer by motion in a circle of the magnitude actually employed in the experiments.

Before discussing further the conclusions to be drawn from the results obtained, it will be well to consider the possible influence of systematic sources of error.

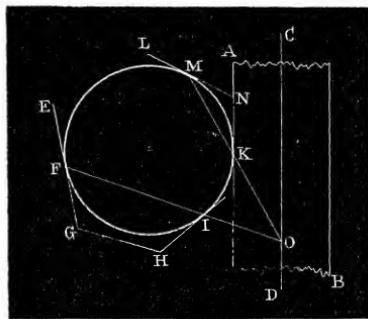
1. *Friction.*—No measure was taken of the amount of friction, nor were any special appliances used to reduce it; the anemometers were mounted in the merry just as they are used in actual registration. Friction arising from the weight is guarded against as far as may be in the ordinary mounting, and what remains of it would act alike in the ordinary use of the instrument and in the experiments, and as far as this goes, therefore, the experiments would faithfully represent the instrument as it is in actual use. But the bearings of an anemometer have also to sustain the lateral pressure of the wind, which in a high wind is very considerable; and the construction of the bearing has to be attended to in order that this may not produce too much friction. So far the whirled instrument is in the same condition as the fixed. But besides the friction arising from the pressure of the artificial wind, a pressure which acts in a direction tangential to the circular path of the whirled anemometer, there is the pressure arising from the centrifugal force. The highest velocity in the experiments was about 30 miles an hour, and at this rate the centrifugal force would be about three times the weight of the anemometer. This pressure would considerably exceed the former, at right angles to which it acts, and the two would compound into one equal to the square root of the sum of their squares. The resulting friction would exceed a good deal that arising from the pressure of the wind in a fixed anemometer with the same velocity of wind (natural or artificial), and would sensibly reduce the velocity registered, and accordingly raise the coefficient which Dr. Robinson denotes by  $m$ , the ratio, namely, of the velocity of the wind to the velocity of the centres of the cups. It may be noticed that the percentages collected in the table on p. 179, are very distinctly lower for the moderate velocities than for the high velocities. Such an effect would be produced by friction; but how far the result would be modified if the extra friction due to the centrifugal force were got rid of, and the whirled anemometer thus assimilated to a fixed anemometer, I have not the means of judging, nor again how far the percentages would be still further raised if friction were got rid of altogether.

Perhaps the best way of diminishing friction in the support of an anemometer is that devised and employed by Dr. Robinson, in which

the anemometer is supported near the top on a set of spheres of gun-metal contained in a box with a horizontal bottom and vertical side which supports and confines them. For vertical support, this seems to leave nothing to be desired, but when a strong lateral pressure has to be supported as well as the weight of the instrument, it seems to me that a slight modification of the mode of support of the balls might be adopted with advantage. When a ball presses on the bottom and vertical side of its box, and is at the same time pressed down by the horizontal disk attached to the shaft of the anemometer which rests on the balls, it revolves so that the instantaneous axis is the line joining the points of contact with the fixed box. But if the lateral force of the wind presses the shaft against the ball, the ball cannot simply roll as the anemometer turns round, but there is a slight amount of rubbing.

This, however, may be obviated by giving the surfaces where the ball is in contact other than a vertical or horizontal direction.

Let AB be a portion of the cylindrical shaft of an anemometer; CD, the axis of the shaft; EFGHI, a section of the fixed box or cup containing the balls; LMN, a section of a conical surface fixed to the shaft, by which the anemometer rests on its balls; FIKM, a section of one of the balls; F, I, the points of contact of the ball with the box; M, the point of contact with the supporting cone; K, the point of



contact or all but contact of the ball with the shaft. The ball is supposed to be of such size that when the anemometer simply rests on the balls by its own weight, being turned perhaps by a gentle wind, there are contacts at the points M, F, I, while at K the ball and shaft are separated by a space which may be deemed infinitesimal. Lateral pressure from a stronger wind will now bring the shaft into contact with the ball at the point K also, so that the box on the one hand and the shaft with its appendage on the other, will bear on the ball at four points. The surface of the box as well as that on the cone LN being supposed to be one of revolution round CD, those four points will be

situated in a plane through CD, which will pass of course through the centre of the ball.

If the ball rolls without rubbing at any one of the four points F, I, K, M as the anemometer turns round, its instantaneous axis must be the line joining the points of contact, F, I, with the fixed box. But as at M and K likewise there is nothing but rolling, the instantaneous motion of the ball may be thought of as one in which it moves as if it were rigidly connected with the shaft and its appendage, combined with a rotation over LNAB supposed fixed. For the two latter motions the instantaneous axes are CD, MK, respectively. Let MK produced cut CD in O. Then since the instantaneous motion is compounded of rotations round two axes passing through O, the instantaneous axis must pass through O. But this axis is FI. Therefore, FI must pass through O. Hence the two lines FI, MK, must intersect the axis of the shaft in the same point, which is the condition to be satisfied in order that the ball may roll without rubbing, even though impelled laterally by a force sufficient to cause the side of the shaft to bear on it. The size of the balls and the inclinations of the surfaces admit of considerable latitude subject to the above condition. The arrangement might suitably be chosen something like that in the figure. It seems to me that a ring of balls constructed on the above principle would form a very effective upper support for an anemometer whirled with its axis vertical. Possibly the balls might get crowded together on the outer side by the effect of centrifugal force. This objection, should it be practically found to be an objection, would not of course apply to the proposed system of mounting in the case of a fixed anemometer. Below, the shaft would only require to be protected from lateral motion, which could be done either by friction wheels or by a ring of balls constructed in the usual manner, as there would be only three points of contact.

2. *Influence on the Anemometer of its own Wake.*—By this I do not mean the influence which one cup experiences from the wake of its predecessor, for this occurs in the whirling in almost exactly the same way as in the normal use of the instrument, but the motion of the air which remains at any point of the course of the anemometer in consequence of the disturbance of the air by the anemometer when it was in that neighbourhood in the next preceding and the still earlier revolutions of the whirling instrument.

It seems to me that in the open air where the air impelled by the cups is free to move into the expanse of the atmosphere, instead of being confined by the walls of a building, this must be but small, more especially as the wake would tend to be carried away by what little wind there might be at the time. On making some enquiries from Mr. Whipple as to a possible vorticose movement created in the air through which the anemometer passed, he wrote as follows:—"I feel

confident that under the circumstances the tangential motion of the air at the level of the cups was so small as not to need consideration in the discussion of the results. As in one or two points of its revolution the anemometer passed close by some small trees in full leaf, we should have observed any eddies or artificial wind had it existed, but I am sure we did not."

3. *Influence of the Variation of the Wind; first, as regards Variations which are not Rapid.*—During the 20 or 30 minutes that an experiment lasted, there would of course be numerous fluctuations in the velocity of the wind, the mean result of which is alone recorded. The period of the changes (by which expression it is not intended to assert that they were in any sense regularly periodic), might be a good deal greater than that of the merry, or might be comparatively short. In the high velocities, at any rate, in which one revolution took only three or four seconds, the supposition that the period of the changes was large compared with one revolution is probably a good deal nearer the truth than the supposition that it is small.

On the former supposition, the correction for the wind during two or three revolutions of the merry would be given by the formulæ already employed, taking for  $W$  its value at the time. Consequently, the total correction will be given by the formulæ already used, if we substitute the mean of  $W^2$  for the square of mean  $W$ . The former is necessarily greater than the latter; but how much, we cannot tell without knowing the actual variations. We should probably make an outside estimate of the effect of the variations, if we supposed the velocity of the wind twice the mean velocity during half the duration of the experiment, and nothing at all during the remainder. On this supposition, the mean of  $W^2$  would be twice the square of mean  $W$ , and the correction for the wind would be doubled. At the high velocities of revolution, the whole correction for the wind is so very small, that the uncertainty arising from variation as above explained is of little importance, and even for the moderate velocities it is not serious.

4. *Influence of Rapid Variations of the Wind.*—Variations of which the period is a good deal less than that of the revolutions of the whirling instrument act in a very different manner. The smallness of the corrections for the wind hitherto employed depends on the circumstance that with uniform wind, or even with variable wind, when the period of variation is a good deal greater than that of revolution of the merry, the terms depending on the first power of  $W$ , which letter is here used to denote the momentary velocity of the wind, disappear in the mean of a revolution. This is not the case when a particular velocity of wind belongs only to a particular part of the circle described by the anemometer in one revolution. In this case there will in general be an outstanding effect depending on the first power of  $W$ ,

which will be considerably larger than that depending on  $W^2$ . Thus suppose the velocity of whirling to be 30 miles an hour, and the average velocity of the wind 3 miles an hour; the correction for the wind supposed uniform, or if variable, then with not very rapid variations, will be comparable with 1 per cent. of the whole; whereas, with rapid variations, the effect in any one revolution may be comparable with 10 per cent. There is, however, this important difference between the two: that whereas the correction depending on the square leaves a positive residue, however many experiments be made, the correction depending on the first power tends ultimately to disappear, unless there be some cause tending to make the average velocity of the wind different for one azimuth of the whirling instrument from what it is for another. This leads to the consideration of the following conceivable source of error.

5. *Influence of Partial Shelter of the Whirling Instrument.*—On visiting the merry-go-round at the Crystal Palace, I found it mostly surrounded by trees coming pretty near it, but in one direction it was approached by a broad open walk. The consequence is, that the anemometer may have been unequally sheltered in different parts of its circular course, and the circumstances of partial shelter may have varied according to the direction of the wind. This would be liable to leave an uncompensated effect depending on the first power of  $W$ . I do not think it probable that any large error was thus introduced, but it seemed necessary to point out that an error of the kind may have existed.

The effect in question would be eliminated in the long run if the whirling instrument were capable of reversion, and the experiments were made alternately with the revolution in one direction, and the reverse. For then, at any particular point of the course at which the anemometer was more exposed to wind than on the average, the wind would tend to increase the velocity of rotation of the anemometer for one direction of revolution of the whirling instrument just as much, ultimately, as to diminish it for the other. Mere reversion of the cups has no tendency to eliminate the error arising from unequal exposure in different parts of the course. And even when the whirling instrument is capable of reversion, it is only very slowly that the error arising from partial shelter is eliminated compared with that of irregularities in the wind; of those irregularities, that is to say, which depend on the first power of  $W$ . For these irregularities go through their changes a very great number of times in the course of an experiment lasting perhaps half an hour; whereas, the effect of partial shelter acts the same way all through one experiment. It is very desirable therefore, that in any whirling experiments carried on in the open air, the condition of the whirling instrument as to exposure or shelter should be the same all round.

The trees, though taller than the merry when I visited the place last year, were but young, and must have been a good deal lower at the time that the experiments were made. Mr. Whipple does not think that any serious error is to be apprehended from exposure of the anemometer during one part of its course and shelter during another.

From a discussion of the foregoing experiments, it seems to me that the following conclusions may be drawn :—

1. That, at least for high winds, the method of obtaining the factor for an anemometer, which consists in whirling the instrument in the open air is capable, with proper precautions, of yielding very good results.

2. That the factor varies materially with the pattern of the anemometer. Among those tried, the anemometers with the larger cups registered the most wind, or in other words required the lowest factors to give a correct result.

3. That with the large Kew pattern, which is the one adopted by the Meteorological Office, the register gives about 120 per cent. of the truth, requiring a factor of about 2·5, instead of 3. Even 2·5 is probably a little too high, as friction would be introduced by the centrifugal force, beyond what occurs in the normal use of the instrument.

4. That the factor is probably higher for moderate than for high velocities; but whether this is solely due to friction, the experiments do not allow us to decide.

Qualitatively considered, these results agree well with those of other experimentalists. As the factor depends so much on the pattern of the anemometer, it is not easy to find other results with which to compare the actual numbers obtained, except in the case of the Kew standard. The results obtained by Dr. Robinson, by rotating an anemometer of this pattern without friction purposely applied, are given at pp. 797 and 799 of the "Phil. Trans." for 1878. The mean of a few taken with velocities of about 27 miles an hour in still air gave a factor 2·36, instead of 2·50, as deduced from Mr. Jeffery's experiments. As special antifriction appliances were used by Dr. Robinson, the friction in Mr. Jeffery's experiments was probably a little higher. If such were the case, the factor ought to come out a little higher than in Dr. Robinson's experiments, which is just what it does. As the circumstances of the experiments were widely different with respect to the vorticose motion of the air produced by the action of the anemometer in it, we may I think conclude that no very serious error is to be apprehended on this account.

In a later paper ("Phil. Trans." for 1880, p. 1055), Dr. Robinson has determined the factor for an anemometer (among others) of the Kew pattern by a totally different method, and has obtained values considerably larger than those given by the former method. Thus the limiting value of the factor  $m$  corresponding to very high

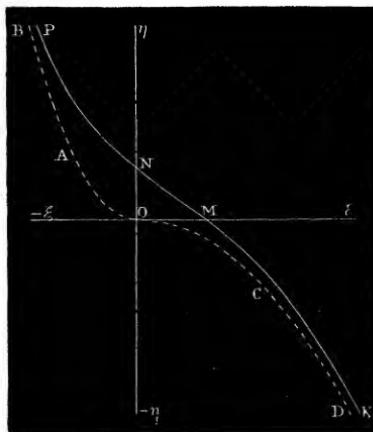
velocities, is given at p. 1063 as 2·826, whereas the limiting value obtained by the former method was only 2·286. Dr. Robinson has expressed a preference for the later results. I confess I have always been disposed to place greater reliance on the results of the Dublin experiments, which were carried out by a far more direct method, in which I cannot see any flaw likely to account for so great a difference. It would be interesting to try the second method in a more favourable locality.

I take this opportunity of putting out some considerations respecting the general formula of the anemometer, which may perhaps not be devoid of interest.

The problem of the anemometer may be stated to be as follows:—Let a uniform wind with velocity  $V$  act on a cup anemometer of given pattern, causing the cups to revolve with a velocity  $v$ , referred to the centre of the cups, the motion of the cups being retarded by a force of friction  $F$ ; it is required to determine  $v$  as a function of  $V$  and  $F$ ,  $F$  having any value from 0, corresponding to the ideal case of a frictionless anemometer, to some limit  $F_1$ , which is just sufficient to keep the cups from turning. I will refer to my appendix to the former of Dr. Robinson's papers ("Phil. Trans." for 1878, p. 818), for the reasons for concluding that  $F$  is equal to  $V^2$  multiplied by a function of  $V/v$ . Let

$$V/v = \xi, \quad F/V^2 = \eta,$$

then if we regard  $\xi$  and  $\eta$  as rectangular co-ordinates, we have to determine the form of the curve, lying within the positive quadrant  $\xi O \eta$ , which is defined by those co-ordinates.



We may regard the problem as included in the more general pro-

blem of determining  $v$  as a function of  $V$  and  $F$ , where  $v$  is positive, but  $F$  may be of any magnitude and sign, and therefore,  $V$  also.\* Negative values of  $F$  mean, of course, that the cups instead of being retarded by friction, are acted on by an impelling force making them go faster than in a frictionless anemometer, and values greater than  $F_1$  imply a force sufficient to send them round with the concave sides foremost.

Suppose now  $F$  to be so large, positive or negative, as to make  $v$  so great that  $V$  may be neglected in comparison with it, then we may think of the cups as whirled round in quiescent air in the positive or usual direction when  $F$  is negative, in the negative direction when  $F$  is greater than  $F_1$ . When  $F$  is sufficiently large the resistance may be taken to vary as  $v^2$ . For equal velocities  $v$  it is much greater when the concave side goes foremost, than when the rotation is the other way. For air impinging perpendicularly on a hemispherical cup, Dr. Robinson found that the resistance was as nearly as possibly four times as great when the concave side was directed to the wind as when the convex side was turned in that direction.† When the air is at rest and the cups are whirled round, some little difference may be made by the wake of each cup affecting the one that follows. Still we cannot be very far wrong by supposing the same proportion, 4 to 1, to hold good in this case. When  $F$  is large enough and negative,  $F$  may be taken to vary as  $v^2$ , say to be equal to  $-Lv^2$ . Similarly, when  $F$  is large enough and positive,  $F$  may be taken equal to  $L'v^2$ , where in accordance with the experiment referred to,  $L'$  must be about equal to  $4L$ . Hence we must have nearly—

$$\eta = -L\xi^2, \text{ when } \xi \text{ is positive and very large} ;$$

$$\eta = 4L\xi^2 \quad , \quad \text{negative} \quad , \quad ,$$

Hence if we draw the semi-parabola OAB corresponding to the equation  $\eta = 4L\xi^2$  in the quadrant  $\eta O - \xi$ , and the semi-parabola OCD with a latus lectum four times as great in the quadrant  $\xi O - \eta$ , our curve at a great distance from the origin must nearly follow the parabola OAB in the quadrant  $\eta O - \xi$ , and the parabola OCD in the quadrant  $\xi O - \eta$ , and between the two it will have some flowing form such as PNMK. There must be a point of inflexion somewhere between P and K, not improbably within the positive quadrant  $\xi O \eta$ . In the neighbourhood of this point the curve NM would hardly differ from a straight line. Perhaps this may be the reason why Dr. Robinson's experiments in the paper published in the "Phil. Trans." for 1878 were so nearly represented by a straight line.

\* Of course  $v$  must be supposed not to be so large as to be comparable with the velocity of sound, since then the resistance to a body impelled through air, or having air impinging on it, no longer varies as the square of the velocity.

† "Transactions of the Royal Irish Academy," vol. xxii, p. 163.



